

dosimetric variables were known, their duplication for a mouse or rat may fail to account for species differences in mass, anatomy, and physiology--and for physical differences in depth of penetration and distribution of absorbed energy. Thus one might challenge the findings of Szmigielski and colleagues, who found reliable evidence of promotion of induced and spontaneous malignancies in mice after a prolonged series of intermittent exposures to a microwave field. The investigators instituted a schedule of exposures that loosely emulate in timing and intensity what some human workers might encounter during a succession of workweeks, but the mouse's three years of longevity and the human being's three score and ten are disproportionate by a factor in excess of 20. A four-hour exposure of a man may be akin to an 80-hour exposure of a mouse.

The criterion of biological equivalence, like that of ecological validity, is difficult to meet. To invoke again the work of Szmigielski and colleagues, the demonstration of accelerated growth of spontaneous mammary cancer was based on a genetically susceptible strain of animal that has been inbred for this deficiency. Does not selection of this strain violate the need for a model that emulates human susceptibility? Some reviewers have answered in the affirmative, and accordingly they have challenged the generality of the finding.

A well-established regulatory convention may well rescue the *in vivo* data from arguments that the criteria of ecological validity and biological equivalence have not been met. Reference is to rules of procedure and interpretation mandated by the U.S. Food and Drug Administration (FDA). The thrust of these rules is that an agent producing toxicity in laboratory animals has *prima facie* weight as a human toxin. Admittedly more of a legislated than a scientific principle, one may nonetheless offer supportive scientific argument. The life spans of man and mouse indeed may differ, but the latter species ostensibly has greater resistance to acute insult by microwave fields at the intensities and doses used by Szmigielski and colleagues. And the charge that a genetically susceptible strain of animal fails to emulate the human condition is valid only for healthy human beings free of hereditary defects. The general population contains thousands of individuals that share with inbred mice a heightened sensitivity to insult. These individuals, too, are deserving of protection.

It is a scientific truism that no datum is a hard datum that establishes a reasonable claim to a causal linkage until confirmed in independent experiment. By this criterion, if strictly interpreted, all the *in vivo* studies under review present soft data in need of confirmation. If more generously interpreted, one can make a case that the findings of Kunz and colleagues represent an extension if not a confirmation of the findings of Szmigielski and colleagues. Despite much lower SARs (0.15-4.0 vs. 3-9 W/kg), Kunz et al. observed a highly reliable difference in the incidence of primary malignant tumors in their aging rats.

The survival times of Kunz et al.'s rats were not diminished by their near-life-long exposure to the field. That the increased incidence of cancer in irradiated rats was not paralleled by reduced longevity poses an interpretive problem that has led some scientists to dismiss the the cancer datum as an artifact of chance. However, although 18 percent of the irradiated rats developed primary malignancies (compared with five percent in controls), the preponderance of animals in both groups died from other causes, and much larger samples might have been needed to detect, statistically, a cancer-related difference in survival times. The SARs to which the rats were subjected were relatively low and were diminishing over time. If the cancer datum is real, and if the SARs were close to threshold values, somewhat higher SARs would have been needed to induce a the higher incidence of malignancies that would be reflected in truncated survival.

As noted, the finding of a high percentage of primary malignancies in the rats of Kunz et al. has been dismissed by some scientists as a quirk of chance. This dismissal is predicated on the differing sites of tumor growth and kinds of tumors, and because the historical data on the normal, untreated Holtzman rats used by Kunz et al. indicate that a high incidence of malignancies is common in the aging animal. The first criticism is valid for a carcinogen or co-carcinogen with a specific affinity for some organ, but it may not hold for an agent that results in non-specific stress. The second criticism is of little merit because historical data cannot control for the vagaries of a particular experimental environment. It is unfortunate, moreover, that the Kunz et al. study did not incorporate reference (normally caged, untreated) controls, as well as an experimental group of rats exposed to continuous-wave (as opposed to pulsed) microwave fields. For therein lies one of the two strong differences in the studies of Szmigielski, et al., and Kunz, et al.: the disparity of SARs, already mentioned, and use in one study of continuous waves vs. use of pulsed waves in the other.

The findings of Kunz et al. cannot be accepted as "hard" data until independently confirmed and extended. But neither can they be dismissed. In the light of the data reported by Szmigielski et al. and by Balcer-Kubiczek and Harrison, who found evidence that microwaves are cancer promoters, and by Manikowska-Czerska et al., who found that pulsed microwaves at very low intensities were associated with chromosomal translocations, there are grounds to suspect that pulsed RF fields at relatively low intensities may have adverse consequences for some segments of the human population.

The reviewer has made much of the factor of corporal restraint as a source of stress artifact in experimentation by Manikowska et al., which also may have been present in the experiments of Manikowska-Czerska et al. Although a confounding factor, the stress of restraint may have numerous "real-world" analogues. That is, healthy human beings may be refractory to insult by RF radiations at intensities low to moderate, but individuals debilitated by disease, by inborn errors of metabolism, or by intense

stress of physical or psychological origin, may be selectively susceptible to RF radiations, even at relatively low intensities. This susceptibility may be heightened when the field is sinusoidally modulated or pulsed at certain critical frequencies.

RECOMMENDATIONS

For reasons already given, threshold power densities or SARs for adverse effects on health are not forthcoming in the epidemiological studies. The highest power density measured at the U.S. Embassy in Moscow, 18 mW/cm^2 , appears to be a "safe" level, given the absence of adverse effects reported for the Embassy employees and offspring. Similarly, power densities at which U.S. Navy personnel were presumed to be exposed to microwave fields in the Silverman et al. study ranged as high as $100 \text{ } \mu\text{W/cm}^2$ --again without evidence of adverse effects. Unknown in both studies are ranges and durations of exposure, which preclude the data from offering a conclusive warrant of safety.

Thresholds of irreversible insult were not determined in any of the *in vivo* or *in vitro* experiments reviewed in this report. From the study of Szmigielski and colleagues emerged convincing but as yet unconfirmed data that microwave fields at $5,000 \text{ } \mu\text{W/cm}^2$ (SARs of 2-3 W/kg) can accelerate growth of spontaneous and induced cancers in mice, which reliably reduced survival times. The data of Kunz et al., which are based on near-life-long exposures of rats at SARs ranging downward from 0.4 to 0.15 W/kg, also are lacking independent confirmation. Longevity was not affected in spite of a more-than-threefold increase in frequency of primary malignant tumors, which may be indicative that a threshold for injury to the rat's immune system lies between 150 and 400 mW/kg . These SARs would scale to power densities approximating 3,500 to 10,000 $\text{ } \mu\text{W/cm}^2$ for human beings exposed in the far zone of a comparable microwave field.

Independent confirmation of the studies of Szmigielski et al. and, especially, of the study by Kunz et al. would augur the need for a new operational definition of the SAR threshold of harm in U.S. standards for limiting exposures to RF fields. Until independent studies are performed that confirm or fail to confirm the tentative evidence of cancer promotion and carcinogenesis--and conduct of such studies should be accorded high priority--there are no grounds in the currently available data base to reduce limits on exposure to RF fields below those recommended or contemplated, e.g., by the NCRP in its Report No. 86.

The most stringent limits recommended by the NCRP are on Very High Frequency (VHF) fields at frequencies between 30 and 300 MHz; to maintain a whole-body averaged SAR below 0.08 W/kg for general-population exposures, the power density is limited to $200 \text{ } \mu\text{W/cm}^2$. (This power-density limit is further reduced if certain modulation parameters are present in incident radiations, but these parameters are not present in the Seattle sources of RF radiation and, therefore, are not at issue.)

Noted in NCRP Report No. 86 is a problem referred to earlier in this review--RF burns and electric shocks--that has not been identified or resolved in earlier reviews or standards published by the ANSI or the EPA. Reference is to recent findings on burn and shock thresholds by Om P. Gandhi at the University of Utah, which have been confirmed in the laboratory of Arthur W. Guy at the University of Washington in Seattle. Based solely on established thresholds of burn and electric-shock hazards, currently, permissible exposures to electric fields in the United States are more than 35 times too high at certain frequencies. (Conversely, limits on exposure to magnetic fields are as much as 10,000 times too stringent at certain frequencies.) Additional research is needed to identify and quantify limits on power densities at which burn and shock hazards are controlled.

All three sets of data--epidemiological, *in vivo*, and *in vitro*--point to the need for experimental studies that evaluate how RF radiations act when combined with toxic (including carcinogenic) agents. Controlled studies on laboratory animals should be performed to determine how thresholds to such agents are altered as a function of SAR, field frequency, and modulation parameters.

Finally, another issue of high priority is the character of fields that are radiated by transmitters of amateur-radio operators. The proximity of radiating antennae to the homes and occupants of the amateur operators, the sophistication of the operators in making measurements of fields in and about the home, and the carefully recorded logs in which are records of frequencies used and times spent "on the air"--all create the scenario for a highly valuable, prospective epidemiological study.

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The Microwave Debate

E200

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Science, Scientists, and Science Policy

The lesson is that we must evolve methods of managing and coordinating research without inhibiting the investigator's freedom. . . . [This] is of particular importance when basic research pertains to a matter of public interest. You must have independent research if you expect to get results that can be believed. At the same time you have to be able to channel the efforts in a direction most likely to produce the necessary results.—Charles Süsskind, Richmond Symposium, 1969

Shortly after it was delegated regulatory responsibilities in 1968, BRH asked a researcher at nearby Virginia Commonwealth University, Stephen Cleary, to help assemble a group of scientists who could assess the present state of knowledge on RF bioeffects. BRH's expectations at the time were clear: "We are . . . very much interested in the total contribution which this Symposium will make to our present knowledge and the guidance you will offer us for future efforts in developing required performance standards for devices that emit unneeded microwave radiation."¹ BRH officials felt that the scientific community could contribute "important value judgments which will make a positive impact on the activities" of government in the "day-to-day administration of . . . Public Law 90-602."²

Scientists traditionally have responded eagerly to government calls for assistance. Since providing advice and receiving support customarily go hand in hand, scientists have been more than willing to help government solve its problems. This was particularly true in the late 1960s. Even before PL 90-602 was passed, former Tri-Service researchers used the occasion of the completion of the last Tri-Service project, awarded to Michael-

son's group at Rochester University, to convene a panel discussion on the "Biological Effects of Microwaves: Future Research Directions" at the 1968 International Microwave Power Institute annual meetings.³ With two consenting partners, it should come as no surprise that RF bioeffects policy of the last decade and a half has been primarily the product of a marriage between scientific research and government (including the military) support.

Objectives versus Priorities

The initial response of the technical experts to the perceived crisis of the late 1960s was mixed. The relaxed attitude of industry was based heavily on years of experience with RF technology, coupled with a considerable dose of skepticism about experiments that pointed to possible low-level effects. This position was succinctly argued in 1968 by an engineer from the Chemetron Corporation: "I started in with a 50-kW transmitter at 19, unshielded and not very far from the antenna. Apart from losing my hair I feel pretty good. I wonder how or what you have been injecting into the organisms so you get these effects?"⁴ Many experts in the late 1960s were comfortable with the overall conclusions drawn from the Tri-Service program and were willing to trust to industry and to the military, the two primary users of RF technology, the task of monitoring bioeffects.

This view, which was principally maintained by persons (not all) in industry and the military, was vigorously contested by most university and government scientists. From a purely scientific standpoint, the research of the 1950s had barely scratched the surface. Researchers had not succeeded in standardizing experimental techniques when the Tri-Service funding ended. They had not looked in depth at special exposure situations, such as pulsed power. This left researchers such as Tufts University eye specialist Russell Carpenter skeptical about past policy decisions: "I am not ready to be comfortable in a microwave field when I am told that the average power is safely below 10mW/cm² but where I am being subjected to peak powers many times that figure. I would prefer not to be there."⁵ The obvious remedy for this situation was more scientific research.

Most scientists who favored more research were willing to

cast their nets broadly. John Howland, who worked with Sol Michaelson, suggested that serious attention be given to "numerous curiosities" such as "the howling of dogs near transmitters, peculiar actions of birds, fatigue and headaches in workers, and other psychosomatic complaints."⁶ When Howland made this recommendation to colleagues in 1968, the chances of pursuing such research looked brighter than they had a year or two before. The pending radiation control legislation suggested that "we are going to see some interest on the part of the federal government in electromagnetic radiation." Interest was an obvious step toward renewed support. Former Tri-Service researcher Charles Süsskind from the University of California, Berkeley, looked for this renewed support "in the very near future."⁷ His prediction proved correct. Shortly after PL 90-602 was passed, funding for RF bioeffects research became available again.

In its rush to get legislation on the books to deal with the perceived environmental crisis of the late 1960s, Congress by and large ignored organizational problems. Many of the scientists who stood to benefit from the research funds that were bound to follow understood that this situation was not satisfactory. Accordingly when the Office of Telecommunications Policy let it be known that it might be interested in helping to coordinate RF bioeffects research, the scientific community responded quickly. By late 1968 Pandora adviser Sam Koslov had gathered together a few colleagues and drawn up plans for an Electromagnetic Radiation Management Advisory Council (ERMAC). Within a few months his plans had been accepted, and ERMAC's first official meeting was convened in March 1969. The principal agenda item at this and most subsequent meetings was the formulation of recommendations for a coordinated RF bioeffects research program.

ERMAC's members wasted little time getting to the heart of the RF bioeffects problem: "There appeared to be general agreement that the first order of business should be the study of the $10\text{mW}/\text{cm}^2$ radiation limit established within the U.S. as compared with the $10\mu\text{W}/\text{cm}^2$ limitation established by the Soviet Union." By the third meeting (June 5, 1969), Koslov was assigned the task of preparing a draft of a document describing ERMAC's aims and objectives. His first draft "generated considerable discussion with resulting constructive comments." A second draft presented to the fifth ERMAC meeting in Sep-

tember 1969 also provoked discussion, and it was completely revised again. Koslov's description of ERMAC's aims and objectives by this time had become a proposal for a coordinated research program. After additional meetings and many exchanges by letter, ERMAC's aims and objectives were finally printed in December 1971 as a *Program for Control of Electromagnetic Pollution of the Environment: The Assessment of Biological Hazards of Nonionizing Electromagnetic Radiation*.⁸

The problem-solving framework ERMAC's program established was unquestionably scientific. Its opening paragraphs juxtaposed a compelling discussion of the problem against "the solution—a research program." The need for more scientific research was sketched out in unmincing terms: "Unless adequate monitoring and control based on a fundamental understanding of biological effects are instituted in the near future, in the decades ahead, man may enter an era of energy pollution of the environment comparable to the chemical pollution of today." Critical areas were singled out for special attention. It was argued that "the consequences of undervaluing or misjudging the biological effects of long-term, low-level exposure could become a critical problem for the public health, especially if genetic, clinical, physiological, and behavioral effects of electromagnetic radiation at power densities below" $10\text{mW}/\text{cm}^2$. The only way out of the dilemma posed by this situation was more research.⁹

From the need for more research, ERMAC's members turned to specific objectives, identifying four areas as demanding the most attention: long-term, low-level studies; epidemiology; research on basic mechanisms; and coordination activities. In combination these four areas covered the main elements of RF bioeffects research, thereby ensuring that a wide variety of projects would be covered. The program was projected to cost \$63,435,000 for the first five years.

Similar broad recommendations emerged from other quarters in the late 1960s and early 1970s. One industrial representative who favored more research, Bell Labs's George Wilkening, summarized the feelings of his colleagues at the 1969 Richmond symposium: "there seems to be unanimous or almost unanimous agreement that one of the things that should be done is to perform repeat insult type experiments at low levels."¹⁰ A simultaneous plea for more epidemiological data was made by Norman Telles of BRH, who estimated that U.S.

researchers had conducted only one extensive eye study and three lesser general surveys that covered fewer than 400 individuals who had had on an average less than three years of exposure.¹¹ Coordination and information on basic mechanisms were universally agreed to be essential. In sum ERMAC's program succeeded in capturing the feelings of the scientific community and in translating them into a document that could be used to establish a broadly based scientific research program on RF bioeffects.

The broad, ideal objectives established in ERMAC's program did not fare well when it came to establishing priorities. Although most researchers agreed in principle that more attention had to be paid to epidemiology, long-term low-dose studies, and the like, they seldom were willing to assign high enough priorities to such studies to get them funded. When push came to shove, the scientists who advised on RF bioeffects research retreated to the attitudes that had led to the dominance of thermal thinking in previous decades. These scientists were at home with precise, controlled experiments; they were not comfortable with effects that lacked causal explanations. This inevitably narrowed the focus of the RF bioeffects research to controlled animal experiments, theoretical modeling, and a concentration on thermal effects. The old way of thinking died hard.

A researcher who typifies this mode of thinking is Sol Michaelson, who has been a constant participant in the advisory process. Like the rest of his colleagues, he was willing to sketch out broad research agendas in the late 1960s to get research funds flowing again. But his underlying formula for judging the best research remained selective: "The biological indicators of microwave response should be easily replicable and the technique of measuring should not require elaborate equipment. Microwave induced biological changes should have a high probability of occurring. The range of experimental values for the parameter selected should be well defined and should have a very poor range of variability. Ideally the range of normal values . . . should be the reference value when evaluating the pool exposure data."¹² Michaelson's thermal experiments provided the ideal model for this type of experiment. The variable—temperature change—was decisive, easily measurable, and relatively quick in occurring. Normal temperatures could be used for establishing a background against which change

could be measured. Michaelson looked for similar crisp rigor in other RF bioeffects research. The degree to which experiments lived up to his expectations determined the extent to which he recommended their being supported.

The manner in which these attitudes came to dominate RF bioeffects research and policy is apparent in the activities of ANSI C95 after the adoption of C95.1 in late 1966. The framer of this first standard, Herman Schwan, began to relinquish control of ANSI activities in November 1965 when he resigned the chairmanship of C95. His place was temporarily filled by the C95 secretary, Glenn Heimer, until a new chairman, Saul Rosenthal of the Polytechnic Institute of Brooklyn, was appointed in June 1968.¹³

Rosenthal took over the chairmanship under difficult circumstances. At the time Schwan was on sabbatical in Germany and had left little information behind on how C95.IV had functioned. The records Rosenthal had were "not complete since there is a jump between 1961 and 1963 from a committee of 14 members headed by Colonel Knauf to a committee of 5 members headed by Dr. Schwan. In addition there seems to have been an involvement with the University of Miami and that seems to have disappeared also. In 1963 there appears some information on the proposed standard; however, there was no background information on how it was arrived at."¹⁴ This situation was not rectified until the summer of 1970, when Schwan began attending C95 meetings again and set out for colleagues his account of the standard-setting process.¹⁵ In the meantime Rosenthal had to begin reassessing the RF bioeffects problem, working under the deadline of ANSI's requirement that its standards be reevaluated every five years.

The initial reassessments undertaken during the first few years of Rosenthal's chairmanship singled out two key questions that needed to be addressed. First, the sketchy information available to C95 members left little doubt that C95.1 was based heavily, if not exclusively, on data collected prior to and during the Tri-Service era. One question that had to be answered was whether there was any new information that would require changing the standard. Second, even if no new data had surfaced, ANSI members still had to ask whether the data used to set C95.1 were adequate. If they were not, provisions would have to be made for undertaking additional research.¹⁶

ANSI thinking on both questions was sketched out briefly by

Rosenthal in a letter to BRH director John Villforth, commenting on BRH's proposed microwave oven standard. Villforth obviously needed to keep abreast of ANSI deliberations since conflicts between BRH and ANSI standards could cause problems. In reassuring Villforth that ANSI had not changed its mind and that the proposed oven standard seemed reasonable, Rosenthal predicted that "C95 will find that no additional information has become available since the present standard was adopted that would indicate a change is warranted." His answer to the first question was that scientific research had not discovered anything that invalidated the 10mW/cm^2 standard. But that did not mean that the data base used in setting C95.1-1966 was adequate. Rosenthal characterized this standard, somewhat paradoxically, as "an excellent one [that] still leaves much to be desired" because its data base was "deplorable." The obvious conclusion that followed was that "unless there is a vigorous and active program of research directed toward obtaining the pertinent information," one could not be sure of the validity of the standard.¹⁷

To rectify this situation, ANSI, like ERMAC, began planning for future research from a broad-based perspective. Shortly after Arthur W. (Bill) Guy took over the chairmanship of C95.IV in July 1970, five study groups were set up "to identify and document the requirements for additional information needed to modify or improve present standards": Near-Zone Field Effects, chaired by John Osepchuk (Raytheon); Frequency Effects, chaired by Albert Kall (Ark Electronics) and Sidney Kessler (U.S. Information Agency); Low-level (Athermal) and Modulation Effects, chaired by Allan Frey (Random-line); Environment, chaired by Bill Mumford (Bell Telephone); and Population Groupings, chaired by William Mills (BRH).¹⁸ The coverage being recommended was comprehensive, but the majority of ANSI members were not willing to take steps that would have provided incentives to expand the base of RF bioeffects research. Instead they retreated to the safe world of controlled experiments and thermal thinking, ignoring the consequences this had on their assumed philosophy of standard setting.

One person tried to change this situation, a figure who was himself becoming a source of controversy—Milton Zaret. In a brief but provocative open letter to ANSI members written in April 1970, Zaret recast the language and philosophy of C95.1

in an effort to place a greater burden of proof on those who believed it was adequate. Since general population studies had not been conducted, Zaret proposed that the standard state that "the recommendations are not intended to apply to the general public." Similarly he recommended that pulsed radiation with peak powers more than one hundred times their average and nonuniform fields be excluded, again reflecting the lack of data available. To ensure that other potential problems were not missed, Zaret suggested requiring that "when a radiation generating system either is capable of exceeding the recommendations or is not adequately defined by this guide, then . . . the user should ensure its safety by performing appropriate biological assay experiments." And to avoid providing an image of certainty where none existed, Zaret's final recommendation would have inserted probability into the entire standard by changing the phrase explaining the safety of below threshold exposures from "will not" to "is believed not to result in any noticeable effect to mankind."¹⁹

Had Zaret's proposed reworking of C95.1 been accepted, it would have changed ANSI's philosophy of standard setting and thereby the accepted protocol for RF bioeffects research. The military and other users would have been compelled to pursue general population studies or tell exposed populations that they did not have the evidence to guarantee safety. Industry would have had to run biological assay tests on RF equipment before subjecting workers to it, not after. ANSI would have had to be sure of its scientific information before issuing a firm standard. By shifting the burden of proof, Zaret's proposal would have made long-term, low-dose, and epidemiological studies a necessity.

Zaret fully understood the consequences of his suggestions: "The effect of the peak power reduction to a level justifiable by data would be simply to shift the responsibility for personnel safety to the user, that is, to the military; to *remove* the sanction the document now offers to the use of high pulsed power radars, and thus force the user to justify his safety codes of practice by supporting or engaging in appropriate research."²⁰ Industrial representatives, such as John Osepchuk and Paul Crapuchets (Litton Industries) also understood the consequences of the proposed changes and argued vigorously against them: "Most military radars have duty cycles near .001, or peak powers near 1000 times average, so that the revised

document would no longer apply. Also, if consumer products utilizing microwave power became widely used, the document would not apply there either. So where *would* it apply, and wouldn't this restrict the document into uselessness?"²¹ Zaret was unmoved by such seemingly pragmatic arguments. If the military had to change its defense plans as a result, so be it; but necessity, at least not Zaret's version of necessity, did not rule the day.

Zaret's proposal for revising C95.1 was circulated, criticized, and rejected. In rejecting Zaret's suggestions, ANSI members permitted their recommended research program to remain squarely in the tradition set in the 1930s and followed after World War II. The final report of ANSI's study groups, "Research Needed for Setting of Realistic Safety Standards" (August 1, 1972), mentioned, but suggested no practical means for resolving, the old problem of occupational versus general population exposure.²² The least developed sections of the report were those that dealt with population groupings and the environment. This all but assured that in the years to come, little serious attention would be given to epidemiological studies or long-term, low-level experiments. Instead the dominant notes struck in the study group report were measure, calculate, and replicate, mostly within the context of animal experiments and for the purpose of learning more about basic mechanisms. This is the course most RF bioeffects research would follow throughout the remainder of the 1970s and into the 1980s.

Inconsistencies and Biases

The emphasis placed on controlled animal experiment was not without justification. Knowledge of the precise mechanism for RF-tissue interaction could be useful when making decisions on exposure standards. This is precisely the sort of information BRH and others were seeking in the late 1960s when they turned to the scientific community for help.

The research plan adopted in the 1970s, however, had internal problems. To begin with, few persons asked whether the research goals that had been established were achievable. Time is needed to solve complex scientific problems. In this particular case the possibility existed that RF bioeffects might be dependent on frequency and on tissue type, thus opening the way for thousands of different interactions. In addition RF bioef-

facts researchers were studying operations within the body that themselves were not fully understood. Given these complications, there was no guarantee of success. The \$63 million being asked of government could have been a down payment on a product that could not be delivered. Recognition of this possibility in the early 1970s might have prevented some of the apologizing that had to be done in the late 1970s when Congress began to wonder why the problem had not been solved.

More important, the product that the scientific community has to offer, scientific information, depends heavily on the integrity of the scientific process. Scientific information is reliable only if the methods used to derive it and the scientists who interpret it are reliable. Scientific facts do not emerge apart from process.

The scientific process followed in the RF bioeffects field has been reliable for the most part. Researchers have subdivided the main problem—how RF radiation affects living tissue—into subproblems, designed experiments to solve the subproblems, conducted these experiments, and thereby assembled a significant body of information on RF bioeffects. But the research process has not been above reproach. Over the years individual biases and inconsistencies have raised questions about the integrity of the entire field. Some examples follow.

Radar Death

In March 1954 a forty-two-year-old man walked in front of a transmitting radar installation. Within seconds he felt internal warming and quickly moved. Thirty minutes after the initial exposure he experienced "acute abdominal pain and vomited." An hour later he was admitted to a hospital and within six hours had his appendix removed. Recovery from the operation did not proceed normally. Eleven days later the patient died of complications. A possible cause of death according to John McLaughlin, an attending physician, was "tissue destruction . . . from microwave radiation (radar)."²³

McLaughlin's diagnosis rested on several considerations. During the first operation physicians observed that "the entire parietal and visceral peritoneum were dusky red and the portion of the small bowel that could be seen was beefy in color," indications that the organs in the patient's body cavity could have been "cooked." Moreover the gross appearance and progress of this patient's illness seemed to follow patterns seen in

conclusions have the weight of science behind them. Whatever his motivations the fact remains that Michaelson's scientific analysis of the radar death case was not reliable. Insofar as he has been willing to go beyond fact and interject his own interpretations into the scientific process, his credibility and the credibility of those who support him has legitimately been called into question.

This does not mean that the 1954 accident was a case of radar death. The exposure may have been unrelated to the death; it may have been a secondary causal factor, aggravating a preexisting minor case of appendicitis, or it may have been instrumental in the death. Until more information is collected, this case will remain unsolved. To argue otherwise is not consistent with rigorous scientific thinking.

Epidemiology

Lack of critical scientific thinking has also played a major role in shaping attitudes toward epidemiological studies, surveys of the health of human populations. In principle epidemiological studies are deceptively simple, making it difficult for the public to understand why they have been so neglected. Their main objective is to discover whether select populations that have been exposed to some factor, such as RF radiation, are (retrospective) or will be (prospective) as healthy as similar populations that have not been exposed. If such studies are carefully controlled so that there is only one major variable, then differences in health can be linked to that factor or, conversely, the lack of differences can be used to suggest safety.

In practice epidemiological studies are never this straightforward. Working populations are usually exposed to many variables. Their exposure to any one variable, such as RF radiation, is difficult to quantify. It is difficult to identify stable populations that are large enough to permit the detection of subtle effects. Surveying the health of large populations is time-consuming and expensive. The detection of a moderate effect that takes fifteen years to develop could require monitoring the health of thousands of persons for half their lifetimes.

Despite these difficulties, it has long been recognized that the health of populations routinely exposed to higher than normal amounts of RF radiation should be monitored. Such studies were recommended and carried out at the height of World War II. Epidemiological studies were singled out as important at the

1953 navy conference. The need for more epidemiological studies was included in ERMAC's 1971 program, in a 1978 report prepared for the Office of Science and Technology Policy, and in more recent surveys of ongoing research needs.

These calls for more epidemiological studies have not been followed for the most part. In all only about a dozen such studies have been undertaken in the United States over the past forty years.³² In addition U.S. researchers have been unwilling to accept the epidemiological data collected in the USSR and East European countries in the course of monitoring the health of workers. The fact that workers in these countries who have been exposed to RF radiation often complain of headaches, dizziness, loss of memory, and other asthenic conditions has not been regarded as significant in the United States, primarily because of the uncontrolled manner in which this information has been collected and reported. Philosophically U.S. scientists have not been prepared to believe that alleged RF bioeffects exist until they can be unambiguously tied to RF exposure.

There has been inconsistency in this attitude, however. U.S. researchers have been most demanding when deciding whether effects tentatively identified in epidemiological studies are significant. Typically the research pattern followed has been to keep checking and rechecking until possible causes for concern have been dismissed in some way. Tests conducted by the navy during World War II were ended when a superficial analysis of the data suggested no major effects. A mid-1950 survey of Lockheed workers was repeated when blood abnormalities were found and terminated when the abnormalities supposedly were traced to an experimental error. An ambitious eye screening program begun during the Tri-Service era was ended when it was concluded that the differences that were found—a higher percentage of minor lens defects in exposed workers—were of no clinical significance. A mid-1960s birth defect study conducted in the Baltimore area was rerun when correlations between radar and Down's syndrome emerged; the study was terminated when the correlation disappeared on closer statistical analysis. A similar course of events took place following initial reports of high numbers of congenital abnormalities in the Fort Rucker area, also in the late 1960s. Two ambitious studies undertaken in the late 1970s, one surveying a population of Korean War veterans and the other looking for possible adverse effects among Moscow embassy personnel, were both

terminated when the initial results turned up no apparent correlations between exposure and effects.³³

In all of these studies critical analysis uncovered reasons to question the possible effects discovered, leading to the conclusion that epidemiological studies had not proved that exposure to RF radiation is hazardous; however, critical analysis did not then continue in an effort to determine whether conclusions could be reached about safety. Although the researchers who have conducted the epidemiological studies have consistently pointed out weaknesses in their work that could have allowed effects to slip through undetected, these weaknesses have not been given the same attention as the shortcomings in the methods used to discover effects. The most recent example of this inconsistent use of scientific rigor can be found in a 1976–1978 epidemiological survey of the health of the population exposed to the Moscow signal.

Shortly after the Moscow embassy problem became public in early 1976, State Department officials decided to quell public fears by asking an independent researcher, Dr. Abraham Lillienfeld of Johns Hopkins, to survey the Moscow population for possible health effects. From the start Lillienfeld's study had problems. Personnel lists were difficult to assemble. (The State Department does not publish embassy directories in sensitive areas.) Government agencies such as the CIA and Department of Defense were reluctant to provide information on their personnel. Data on the signal were not made available so those having the highest exposure could not be isolated. Embassy personnel were slow in responding to questionnaires, yet the State Department insisted that a rigorous time schedule be maintained. In all, the Foreign Service Health Status Study (FSHSS), as the Moscow survey came to be called, did not proceed under ideal conditions. Even so data were collected.

The FSHSS data confirmed the State Department's contention that the Moscow signal was not causing immediate health problems. Embassy personnel apparently were not dying any faster or contracting more illnesses than their control counterparts (personnel in other East European embassies). But it became apparent that the study was not sensitive enough to discover anything but immediate major effects. No conclusions could be drawn from the discovery that "the proportion of cancer deaths was higher in female employees" because of the small number of deaths. The difference between exposed and

controlled populations was about two deaths. Similarly no long-term conclusions could be drawn because "the group with the highest exposure to microwaves, those who were present at the Moscow embassy during the period from June 1975 to February 1976, had had only a short time for any effects to appear." This study, like so many other epidemiological studies, was not sensitive enough to allow firm conclusions to be drawn about effects or safety.³⁴

Such weaknesses might have been corrected by improving the scientific process, following the pattern set in studies reporting effects. This step was contemplated by ERMAC members in mid-1981 and rejected. The population and the exposure information were judged inadequate for pursuing a more detailed study; therefore the data could not be refined, making it intellectually unjustifiable to spend more money on the project.³⁵ Thus ambiguities would remain. Correlations between RF exposure and health effects had not been proved but neither had they been disproved.

Members of the scientific community have not been consistent in reporting the inconclusiveness of the FSHSS to the public. State Department adviser Herb Pollack has routinely relied on the FSHSS to assure audiences that long-term exposure is not harmful. A paper he presented in 1980, after discussing all of the measurements that failed to turn up significant differences, concluded that "no convincing evidence was discovered that would directly implicate the exposure to microwave radiation experienced by the employees at the Moscow embassy in the causation of any adverse health effects as of the time of this analysis." Pollack did not provide his audience with the same critical analysis of this conclusion that appeared in the report itself.³⁶

An even more blatant neglect of qualifying statements can be found in an environmental impact statement (EIS) prepared in the Bainbridge Island uplink case. (The EIS was prepared by scientists and intended to be used by government and the public when making decisions about the proposed uplink facility.) After describing the FSHSS and noting that "researchers found no reliable differences between health status records of the Moscow embassy personnel and the control group," the conclusion was reached that "these findings indicate that there is no danger from continuous exposure to microwave radiation at levels below 15 microwatts/cm²."³⁷ This statement not only

ignores the qualifying remarks included in the FSHSS but also presents conclusions that were not drawn and could not have been drawn on the basis of the available evidence.

Such one-sided reporting pervades other portions of the discussion of epidemiological studies in the Bainbridge Island EIS. After expressing the usual methodological reservations about USSR and East European research, the authors of the EIS accept without question a recent summary of USSR and East European epidemiological studies delivered in absentia by a Czechoslovakian researcher in 1981: the relevance of the summarized studies is that they are said to "imply that there is no danger to [sic] continuous exposure to microwave radiation levels of $10\mu\text{W}/\text{cm}^2$ and below." This in turn supports the contention that "there are no substantial data from epidemiological studies to suggest that adverse health effects seen in laboratory animals have occurred in human populations."³⁸

Why this particular survey has been accepted when other USSR and East European surveys are rejected is not explained. Neither is it explained why the attention of readers is drawn only to studies that found no effects when the summary mentioned as well studies that found effects. The Czechoslovakian researcher presenting the summary, Jana Pazderova-Vejlupkova, had not observed differences in most health tests run on exposed and unexposed radio station employees, but she did find "a reliable difference in [the] variability of the response to the glucose-tolerance test." Another paper she mentioned reported "no difference in the morphological character of lenticular opacities of exposed and unexposed populations of men," but the same paper offered "the view that long-term exposure to microwaves at power densities below cataractogenic levels can accelerate the natural aging process of the lens." The selective use of data illustrated by these examples hardly seems consistent with a final goal set out in the Pazderova-Vejlupkova paper: "My plea is for adherence to the principle of objectivity."³⁹

Admittedly the Bainbridge Island EIS is a particularly biased document and could be dismissed if it had as quickly been dismissed by the scientific community as McLaughlin's radar death article. But this has not happened. Critics of the way RF bioeffects research has been conducted have offered comments, but not the broader scientific community that wants the public to accept its judgments about RF bioeffects.⁴⁰ While be-

ing rigorous in its criteria for accepting effects or hazards, this community has ignored its own ambivalence toward epidemiological studies and the misuse of them by some of its members. The result of this situation has been a further erosion of the credibility of the scientific community, as viewed from the public's perspective.

Microwave Cataracts

Epidemiological studies and data on individual deaths are controversial by nature; neither can be controlled easily, and neither routinely leads to firm conclusions. The RF bioeffects community is not alone in the problems it has had in dealing with these aspects of science. But the bioeffects debate has not been limited to the softer aspects of science. Controversy has arisen over RF bioeffects that are known to exist, such as the RF or, as it is more commonly called, the microwave cataract.

By the mid-1960s, it was commonly felt that the threshold for cataract formation was about $100\text{mW}/\text{cm}^2$, a figure easily explained using standard thermal mechanisms: exposure at $100\text{mW}/\text{cm}^2$ was known to cause heating, and the eye is known to be particularly susceptible to overheating. The microwave cataract came to be understood as a thermal injury that occurred when the eye is exposed to thermal doses of RF radiation.

Scientific opinion was not unanimous on this subject, however. Russell Carpenter discovered that a single $280\text{mW}/\text{cm}^2$ dose administered to rabbits for three minutes did not produce cataracts. The same dose produced cataracts if administered once daily for three or four consecutive days. It did not produce cataracts if the interval between exposures was extended from daily to weekly, leading Carpenter to suggest that single doses that were not cataractogenic (cataract producing) could produce minor effects that could accumulate and lead to cataract formation if sufficient recovery time were not allowed between exposures. Put more simply, his experiments led to the conclusion that cataracts might be a cumulative RF bioeffect. Subsequent failure to find consistent correlations with temperature rise led to the further suggestion that nonthermal causes might be involved as well.⁴¹

Carpenter's break with the thermal interpretation of microwave cataracts met with opposition. In a review article published in 1972, Michaelson criticized Carpenter's use of the

term *cumulative*. As far as Michaelson was concerned, cumulative damage occurred only when each exposure produced irreparable damage. Carpenter's experiments suggested that the damage was not irreparable since the rabbit's eyes apparently were able to return to normal in a few days, thus explaining the absence of injury with the weekly exposure schedule.¹² A year later, in his review paper submitted to the 1973 Senate hearings, Michaelson made this point more strongly: "It is utterly incongruous that an important concept such as cumulative effect of microwaves should be based on such scanty data. . . . Any scientist can readily see how inappropriate it is to use such inadequate data as a basis for any meaningful analysis." As was his custom Michaelson used the lack of convincing evidence as grounds for total rejection: "Since it has not been conclusively shown, the suggestion of cumulative effects of microwave exposure is untenable."¹³

When Michaelson issued this assessment, more was at stake than Carpenter's cautious excursion into unorthodox thinking. By the time of the second round of Senate hearings, Milton Zaret had advanced a microwave cataract theory that not only challenged orthodox thinking but opened the door to wide criticism of prevailing RF bioeffects policy. In the minds of some, Zaret's views went beyond heterodoxy to heresy and had to be treated accordingly.

The broad outline of Zaret's microwave cataract theory is fairly simple, if somewhat vague. In essence he has repeatedly claimed that long-term, low-level exposure to RF radiation can cause cataracts: "Chronic microwave cataract develops slowly over a period measured in years and follows repeated irradiation at nonthermal intensities; it presents clinically as a gradual degradation of the lens capsule, without any evidence of burn, and resembles a delayed radiational effect."¹⁴ The distinguishing feature of a chronic microwave cataract, in comparison to a thermal microwave cataract, is its location in the eye. Thermal microwave cataracts form in the rear of the lens; chronic (athermal) microwave cataracts, according to Zaret, form in the capsule covering the rear of the lens. The posterior lens capsule, Zaret argues, is gradually clouded by continuous RF exposure, leading to the opacification that characterizes a cataract. The opacification may, in Zaret's view, eventually affect the lens proper, but it begins in the capsule. This distinguishing place of origin, along with some knowledge of prior

health, allows Zaret to recognize a cataract as a chronic microwave cataract.

The response to Zaret's microwave cataract theory was cool at best, and as long as it was not taken seriously, no one seemed particularly concerned. But as soon as his theory was cited as evidence for low-level effects, it quickly became the target of attack. An attempt by Leo Birenbaum in 1972 to use one of Zaret's eye surveys to question Michaelson's contention that to date there had been little information on injury brought forth the familiar elaboration of the shortcomings of such surveys.¹⁵ Two years later a concerted campaign to dismiss Zaret's microwave cataract theory was mounted following the publication of a case study in which Zaret potentially linked cataracts in a fifty-one-year-old woman to an allegedly faulty microwave oven.¹⁶ The case study aroused particular concern in the industrial sector of the RF bioeffects community, which at the time was grappling with the problem of growing public distrust of RF technology. The biased responses that followed again undermined the credibility of the scientific process.

Zaret's 1974 report was severely criticized by Michaelson, Raytheon spokesman John Osepchuk, Budd Appleton of the air force, Russell Carpenter, and Harvard ophthalmologist David Donaldson for suggesting that exposure at levels below the ANSI standard could cause injury. "As to the amount of radiation," noted Donaldson, "the incident power is well within the safe limits as specified by the ANSI C95 exposure standard."¹⁷ Appleton was so convinced of the safety of the standard that he was not concerned even with faulty ovens: "Many people in the United States have purchased microwave ovens; many thousands of these appliances are now being used in American homes. Probably some of them leak in excess of the standard currently used. . . . There is mounting evidence that the standard could safely be raised, and although there is not much pressure to raise this standard, exposures in excess of it are being viewed with progressively less alarm."¹⁸ These arguments had no bearing on Zaret's arguments since he was in part reporting the case as evidence for reconsidering the validity of the ANSI standard. To reject his arguments on the basis of that standard and not on the basis of scientific evidence represented a classic exercise in circular reasoning.

The manner in which Zaret's critics handled the scientific arguments in the 1974 report was not much better. Rather than

trying to understand the case as presented, the facts were distorted and misrepresented in an effort to undermine the connection to RF exposure. Thus, the phrase "her near vision was becoming blurred [in 1961]" was rephrased by Michaelson and Osepchuk as "blurred vision," noting that this symptom appeared five years before the patient had purchased her microwave oven. The point of the critique based on the reworded diagnosis was to suggest that cataracts could have been forming in advance of the purchase of the oven. The point of the original diagnosis, which Michaelson and Osepchuk ignored, was to establish that the woman's eyes were examined and reported to be healthy in 1961 except for the very common condition of nearsightedness.⁴⁹

Michaelson and Osepchuk also faulted Zaret for connecting this case to RF exposure when he was certain that only one of the cataracts was a microwave cataract: "It is incongruous for the author to definitely state that there was a 'microwave cataract' in the left eye, and for the right eye a microwave cataract is presumptive. It seems obvious that, if this were indeed a radiant energy-induced cataract, both eyes without question or neither eye would have had it, unless this particular patient always checked her 'leaky' oven with one eye and not the other."⁵⁰ The logic of this critique is correct; the facts are incorrect. If the patient had uniform exposure, then bilateral cataracts would be expected; however, Zaret's diagnosis of the cataract in the right eye as presumptive in no way suggested that it was not a microwave cataract. He simply did not know whether it was or was not because the cataract had been removed by another physician before Zaret examined the patient. Thus he had to presume rather than conclude, a fact Michaelson and Osepchuk ignored in their version of the case.

Similar biased analyses go hand in hand with legitimate reservations throughout the remainder of the replies to Zaret's 1974 report. Ultimately all of Zaret's critics adopted the traditional position: none was sure what the cause of the cataracts was, but each was sure that the oven was innocent. To ask that scientists supposedly interested in RF bioeffects think further about this case, especially in the light of the fact that cause could not be assigned, was considered a heresy: "the cause and effect relationship [Zaret described] is totally unfounded and represents an erroneous and dangerous conclusion." Zaret was not given the benefit of the doubt even though he listed his

diagnosis as an "impression" and not a "conclusion." No one was willing to take seriously the cautious assessment of New York ophthalmologist George Merriam, which was misleadingly said to oppose Zaret: "As you can appreciate, it is impossible to say with absolute certainty that the case reported was or was not due to microwave exposure."⁵¹

This conclusion can basically be applied to the general state of Zaret's microwave cataract theory. Claims are frequently made that other physicians have looked at some of Zaret's patients and been unable to see the so-called capsular and incipient capsular cataracts. Such checks have never been run under controlled conditions, and the results have not been published. The negative results simply circulate as folklore among those who are convinced that Zaret is wrong. The air force did conduct an eye survey in the early 1970s, but the published accounts of these studies, which claim no effects, are too vague to allow them to be used for evidence.⁵² Zaret's work has been tested in the courts by lawyers⁵³ but not in the laboratory by scientists, a fact that compelled the authors of the Bainbridge island EIS to conclude in the midst of their criticism of Zaret's work under journalistic coverage: "There is still no consensus on the validity of Zaret's theory."⁵⁴ Such consensus will not be forthcoming until the biases against Zaret are dropped and his work accepted or rejected on the basis of objective, scientific analysis.

Politics of Science

These instances of biases and inconsistencies reflect a deep-seated problem that plays a major role in scientific development: the politics of science. Scientists are not immune to social, economic, and political pressures. Gaining support for research, getting articles published, receiving promotions, getting elected to important offices, being asked to advise on government panels, to testify at hearings, or to consult on legal cases—all of the activities of science other than working in the laboratory and thinking—bring scientists face to face with many different pressures. They must live up to the standards set by peers, meet the expectations of employers, be able to gain national and international respect, and so on. As long as scientists and society take care to keep the larger system that supports science from influencing the way science is conducted,

politicization is not a major problem. When care is not taken to keep politics out of science, biases, inconsistencies, and other detrimental consequences can result. Two additional examples from RF bioeffects research illustrate how external pressure can be brought to bear on science.

Long-Term, Low-Level Effects

In September 1978 Bill Guy of the University of Washington signed a contract with the air force agreeing to plan a long-term, low-level RF bioeffects experiment.⁵⁵ Despite persistent calls for such studies, none had previously been conducted in the United States, leaving the air force in a difficult situation. Radar installations are sources of long-term, low-level exposure. By the late 1970s the public was more and more demanding that the air force explain how it could be sure that its radar facilities were not hazardous if long-term, low-level studies had not been conducted. In response the air force made the decision to fund such a study and turned to Guy for help.

The expense and uniqueness of the projected study placed special demands on the scientific process. The full project, from planning through experimentation to final reports, was slated to run about six years and cost close to \$2 million. Plans called for running over thirty tests on 200 rats (100 exposed and 100 control) from shortly after birth to death.⁵⁶ Given the limited resources available for RF bioeffects research, the likelihood of a similar test being run in the near future was remote. Therefore this experiment had to be as rigorous as possible and above criticism if it were to be of any use. To spend this extraordinary amount of money—ten times the amount for surveying the health of the 4000 employees in the Moscow embassy study or \$10,000 per rat compared to \$100 per State Department employee—on an experiment that had flaws would have been a tragic mistake.

By late 1979 Guy's research team began reporting on the experimental procedures that were to be adopted in the full 200-rat study (phase II). Their plans raised questions. It was well known by this time, as Guy himself had argued, that behavioral measures were the most sensitive indicators of RF bioeffects and yet he made no mention of behavioral measures when he published a plan for the full study in the January issue of the *IEEE Proceedings*. Since this article reportedly described all of

the biological end points that would be used in phase II, the conclusion seemed to follow that behavioral measures were being ignored.⁵⁷

This omission troubled independent researcher Allan Frey, who had been studying behavioral and neurological RF bioeffects for over twenty years. To find out more about Guy's study, Frey wrote to an official at the National Telecommunications and Information Administration, Robert Frazier. As executive secretary of ERMAC, Frazier was usually well informed on current research developments. The information he sent in reply did not mitigate Frey's concerns. On June 9, 1980, Frey responded with a detailed critique of the proposed long-term, low-level study, calling into question the motivations of key members of the RF bioeffects community.

Frey objected to more than the absence of behavioral tests; he was concerned about the proposed use of pathogen-free animals. A unique population would not allow extrapolation "to the general population of rats, much less man." He regarded the "minimum stress" argument used to justify the use of pathogen-free animals as "a joke": "Can they really believe that a rat catching a cold, etc., is more stressful than technicians repeatedly sticking needles into the animals to get blood samples?" Frey was not convinced that the frequency being used was appropriate. And he felt that little would be learned about two important measures, nervous system function and immunology. Most important he objected to the fact that the strengths and weaknesses of Guy's study had not been debated by the scientific community: "Is there any wonder some members of the media believe that there are conspiracies afoot? Won't our scientific community's lack of protest be construed as tacit complicity in a conspiracy? What can we as members of the scientific community offer as a defense—the Nuremberg Defense? If so, we'll also (figuratively) hang!"⁵⁸

Such criticisms did not produce open debate; planning for the full 200-rat study remained under the control of Guy's lab and his air force sponsors. But changes were made in the research design as the study progressed. The request for proposal issued by the air force prior to granting the contract for the second experimental phase of the project listed behavior and evaluation of the immune system as two "parameters to be measured."⁵⁹ Both measures were included in the full study, which began on September 1, 1980.

Frey still had reservations. The behavioral study included in the full experiment was an open-field test, a straightforward measure of routine activity. Every six weeks the rats were to be placed, one by one, in a cage designed to measure movement. The cage was divided into squares, each with a photoelectric detection system attached. As the rats moved from square to square, they disturbed the photoelectric system. Each monitored disturbance was fed into a computer and recorded as a measure of the rat's activity. Comparisons of the total movements of exposed versus control rats during one three-minute test session every six weeks formed the data that were used for making judgments about possible behavioral effects.⁶⁰

The addition of the open field test raised new concerns in Frey's mind. At an October 1982 conference I organized to discuss the applicability of risk-benefit analysis to the RF bioeffects field, Frey argued that the open field test was one of the least sensitive behavioral measures that could have been chosen, a view shared by another conference participant, Rochelle Medici, who specialized in studying behavioral effects. If this were the case, then the questions about research ethics remained. Was it unreasonable for the public to believe, Frey asked, "that microwave bioeffects research is at present being channeled to look in the wrong place for effects and . . . that the decisions that have led to this state of affairs were not made in the spirit of science?"⁶¹ Was politics entering into RF bioeffects research?

Guy's response to Frey did not resolve the ethical problems raised. In denying that Frey's criticisms had played a role in shaping the final study, Guy stated that he "did not select the endpoints" for his study. "These were selected in the . . . statement of work contained in the Request for Proposals (RFP) disseminated by the Air Force." In other words Guy deflected Frey's criticism by shifting the responsibility for planning from himself to his air force sponsors. In Frey's view this did not absolve Guy from responsibility. "Why," Frey queried in a follow-up comment, "did Guy take on a project which involved the expenditure of approximately \$1.5 million of public funds with the known critical tests ruled out by the sponsor . . . ? Is this science?"⁶²

Guy did not agree that known critical tests had been omitted from his study. From his perspective "the behavioral endpoint most sensitive to low level chronic microwave exposure, as re-

ported by Professor Shandala, who is responsible for the Soviet microwave exposure standard for the general population, was incorporated into the research plan."⁶³ This claim is debatable. There are significant differences between Guy's work and the experiments of his Soviet colleague.⁶⁴ But even if this problem were overlooked, Frey's basic question still remained: Was it appropriate for the agency seeking scientific advice to select the end points that were used in the study?

The formative role the air force played in Guy's study is not an exception. During the Tri-Service era most decisions about the future of RF bioeffects research were made by the military or within contexts overseen by the military. Much the same support structure was retained in the 1960s, adding the State Department and intelligence communities. Even after the injection of public interests in the late 1960s, the majority of support for RF bioeffects research still came from the military. Throughout the 1970s approximately two of every three research dollars spent on RF bioeffects research can be traced to the navy, air force, or army.⁶⁵ And with this support inevitably came control.

Blood-Brain Barrier Effects

In 1975 Frey advanced the hypothesis that low-level RF exposure (below 10mW/cm²) can disturb the barrier that regulates the exchange of substances between the blood and brain tissue. This blood-brain barrier is critical for keeping the brain's environment stable and operating normally. His hypothesis was based on a deceptively simple experiment in which anesthetized rats were irradiated with low-level RF radiation (CW and pulsed), injected with a fluorescein dye, and sacrificed. The brains of the rats were then sectioned and examined under ultraviolet light for fluorescence. Comparisons of exposed versus control rats turned up significantly more fluorescence in the brain sections of the exposed rats, a result that led Frey to conjecture that low-level RF exposure might cause the blood-brain barrier to leak.⁶⁶

Additional work on this effect was soon carried out by other researchers, and preliminary reports tentatively confirmed Frey's original hypothesis. A George Washington University researcher, Ernest Albert, using horseradish-peroxidase protein tracer instead of fluorescein, found that 10mW/cm² radiation caused increased barrier permeability in both rats and

hamsters. Kenneth Oscar and Daryl Hawkins, two army biomedical researchers, used radioisotope techniques and came up with similar results. Thus by mid-1977, the blood-brain barrier effect had tentatively been confirmed through three fairly independent lines of research.⁶⁷

The reported discovery of this effect met with mixed reaction. Basic research scientists, such as Frey and Albert, were interested in the effect as a possible explanation for behavioral effects. Frey was clearly searching for mechanisms to explain behavioral effects when he began his work. However, from a policy standpoint, barrier effects posed problems, particularly if they occurred at exposure levels below 10mW/cm². Changes in the barrier, or in related phenomena such as blood flow in the brain, as a result of exposure at a presumed safe level could not be dismissed as trivial.

The process of assessing these consequences and deciding what to do began in military circles. In April 1977 a secret conference was held at the U.S. Naval Academy in Annapolis, Maryland, to discuss "undesirable electromagnetic effects." At this conference a research team at Brooks Air Force Base in Texas headed by James Merritt reported finding "significant leakage of fluorescein into the brain substance" of rats after they were exposed to a 50 millisecond dose of high-level RF radiation. The brief, high-level exposure delivered enough energy to the brain, Merritt contended, "to increase brain tissue temperatures significantly" and cause "behavioral and anatomic changes." Whether these tests were designed to replicate any existing or projected exposure situations was not indicated in the unclassified abstract of Merritt's paper that was made public.⁶⁸

Several months later Merritt reported on another aspect of his work at a special session on the blood-brain barrier effect convened during a symposium on the Biological Effects of Electromagnetic Waves. In addition to his high-level experiment, Merritt had also attempted to replicate the work of Frey and Oscar and Hawkins. In both cases he failed to get similar results. At low-level exposure his control and exposed animals exhibited similar barrier leakage. The only differences Merritt found were ones produced by heating test animals in an oven to raise body temperature or after injecting the animals with a substance (urea) known to produce a barrier effect. These results led him to conclude that Frey had probably seen a thermal

effect on the blood-brain barrier produced by exposure levels that were much higher than had been reported.⁶⁹

The conflicting negative and positive reports on low-level barrier effects prompted the navy to convene a third conference to discuss this problem in October 1978. Once again an effort was made to review all prior work for the purpose of making decisions on the future of barrier research. All of the usual participants were present—Frey, Albert, Oscar, Merritt, and others who had conducted related research. The format called for the presentation of papers, discussion, and then a final summary by Don Justesen, a psychologist at a Kansas City Veterans Administration hospital, who was given this important task even though he personally had not done any blood-brain barrier research. A year earlier Justesen had been asked by a committee of the National Council on Radiation Protection, chaired by Guy, to prepare an analysis of the barrier literature to be used for deriving safety standards. He had also inserted in a special issue of *Radio Science*, which he edited, comments on papers given at a panel session on the barrier. Now he was being asked to comment on yet another summary effort, sponsored this time by the navy.⁷⁰

By this time Justesen had seen enough of Merritt's work to have serious reservations. Shortly after the October 1978 navy conference, he subjected Merritt's 1977 symposium paper, which had been published in *Radiation and Environmental Biophysics*, to a careful reanalysis. He concluded that "there is some discrepancy between your [Merritt's] data and your interpretation of same." Where Merritt had found no effects, Justesen reanalyzed and found them. He also assembled evidence for causal explanations that Merritt had passed over. These objections and others were carefully outlined in a three-page letter written on January 30, 1979. At the time Justesen was under a deadline to produce a review on the blood-brain barrier controversy, forcing him to give Merritt a week to reply.⁷¹

In theory Justesen's critique of Merritt's work left the controversy unresolved. In practice, however, politics entered the debate and attempted to resolve what science could not. Justesen tempered his private criticisms considerably when he took to print. In a comment on the 1977 symposium papers, which finally found their way into print in late June 1979, Justesen reported that Merritt had discovered his own mistakes, whereupon he "reanalyzed his data via a powerful analysis-of-

variance technique and . . . found reliable results." The reported reanalysis also led to several points of interest that bore a remarkable similarity to Justesen's own reanalysis sent to Merritt in January 1979. A year later in a review of the blood-brain barrier controversy, Justesen completely ignored the errors in Merritt's only published paper and presented his own (Justesen's) reinterpretations as though they were Merritt's. This was during the same period that Justesen was wondering in private how Merritt was "going to handle the flat negative conclusion in his abstract [of the 1977 paper] in light of the positive findings obvious in his data."⁷²

Justesen was not alone in trying to brush aside the shortcomings of Merritt's work. The final report on the navy's 1978 blood-brain barrier workshop summarized Merritt's work in two brief paragraphs. The first described his experiment. The second noted the negative findings and added, "Merritt's data has [sic] since been analyzed by Justesen and the conclusions are being restudied."⁷³ More recently a review of blood-brain barrier research prepared at the Stanford Research Institute under an air force contract summarized Merritt's article without any mention of its shortcomings. The same work that Justesen privately took apart piece by piece in his 1979 letter to Merritt had quietly become an example of "more objective and refined detection methods" than were found in Frey's experiments.⁷⁴

The objectives of the preferential treatment afforded not only to Merritt's work but to other studies that have cast doubt on Frey's initial experiments are clear. Justesen's reanalysis endeavored to link the obvious barrier effects Merritt found to thermal mechanisms. Presumably once ties to temperature elevation in the brain were established, the hazards issue was resolved since heat stress was not considered an unusual problem. Viewed from a thermal point of view and using Justesen's estimates, the thermal stresses produced were "no greater than those produced in an animal by the stresses associated with swimming, moderate fasting, learning to escape from an annoying stimulus, or being gentled in the hands of a human being."⁷⁵ In essence, it was argued, thermal effects raised no special safety problems. There was no reason to worry about abnormal leakage through the blood-brain barrier if that leakage was caused by heating.

The logic that lay behind this position rested on one debatable assumption—that RF heating is similar to other forms of heating. By the late 1970s it was known that RF energy heats tissues selectively and causes hot spots. The thermal consequences of being exposed to an RF field and being gentled in a hand are not the same. Thus the discovery of possible thermal mechanisms to explain the blood-brain barrier effect was irrelevant to the central question being debated—the relevance of the discovery of a barrier effect at low-level exposure. In addition it was not at all certain how short-term experiments related to long-term exposure. Justesen felt that chronic exposure posed no special problems, but he was not certain. As he wrote Frey, "What will happen after continuous exposure over weeks and months to fields that promote BBB/circulatory changes? That is the big question. My head tells me not much will happen in the way of deleterious effects, but my gut tells me one simply can't speculate away an untested thesis."⁷⁶

Such doubts notwithstanding, the blood-brain barrier controversy was soon speculated away using untested theses. Following the 1978 navy workshop the conclusions that supposedly emerged from the presentations were summarized in a final report. The different results reported by the participants left no doubt "that from a scientific point of view, much more research needs to be done to understand the structure and function of the blood-brain barrier and to evaluate the implications that an increase in barrier permeability would evoke." The state of scientific knowledge was correctly assessed as incomplete. The report then went on to draw a second conclusion: "There appears to be no theoretical or experimental evidence that low-level microwaves that do not raise the brain temperature could be expected to affect the integrity of the barrier."⁷⁷ This second conclusion was the untested and also misleading one. Despite claims to the contrary Frey's work had not been replicated by other researchers. It was not true, as the final report claimed citing a 1977 publication, that "Oscar and Hawkins were unable to replicate Frey's results."⁷⁸ Merritt had not followed Frey's quantification technique; he had used different techniques for measuring fluorescein leakage. And by the time the workshop summary was submitted, Justesen had already critiqued Merritt's work. Moreover the interjection of thermal reasoning into the second conclusion was irrelevant.

Given the state of uncertainty that characterized blood-brain barrier research in 1978, conclusive evidence either for or against the effect could not have been assembled.

The purpose of the second, untested conclusion is more than apparent from a third conclusion that appeared in the original draft of the workshop summary: "Department of Defense funding of research evaluating the effect of microwaves on the blood-brain barrier should be of low priority." This was the justification used to curtail barrier research in the years to come. This open announcement of policy broke with established tradition, however. Most planning in the military is done in closed meetings, and the results are usually not publicized. The break with tradition was quickly recognized by one of those reviewing the conference for the military, who wrote in the margin of the draft version of the workshop summary: "do we really want to say this?"⁷⁹ Apparently they did not; the third conclusion was omitted when the final report was submitted in May 1979.

Frey tried to keep the scientific debate alive by continuing his research and at every opportunity questioning the work of others, an increasingly difficult task. When Justesen failed to publicize his doubts widely, Frey brought the information to the attention of the scientific community in a letter written for publication in the Bioelectromagnetics Society's *Newsletter*. The *Newsletter* editor, Tom Rozzell, at first denied publication, arguing that the piece Frey had submitted was too long. Later he rescinded this decision and allowed the publication to go forward. At the same time, as an official at the Office of Naval Research, Rozzell found himself in the position of informing Frey that his research contract with the navy was to be terminated. In brief, while Rozzell was fostering scientific debate through the *Newsletter*, he was also restricting it by helping to control the flow of vital military research funds.⁸⁰

One year later, in October 1981, Frey ran up against another obstacle when the editor of the Bioelectromagnetics Society's *Journal* and navy employee, Elliot Postow, refused to publish a paper investigating the effects of RF radiation on the blood-vitreous-humor barrier in the eye. The justifications Postow used were one equivocal negative review by Justesen (Russell Carpenter had submitted a favorable review) and his own objections to the "tongue lashing" Frey had given Spackman, Preston, and Merritt.⁸¹ The frank and open criticism that had

become a characteristic of Frey's publications over the years did not fit with the conventional and conservative style that had come to dominate RF bioeffects research. Frey was not playing the research game according to the rules.

Ramifications

The attempts to limit the circulation of Frey's ideas and other potentially controversial aspects of RF bioeffects research have worked to the disadvantage of both science and policy. In refusing to air its own internal disagreements in public, the establishment that controls RF bioeffects research has misled the public and researchers. When criticized by Frey for not pointing out weaknesses in Merritt's work, two researchers who recently published a survey of the state of BBB research could only reply: "No further details [on the statistical inaccuracies in Merritt's work] were included in the reports we read, and so we left it to the reader to decide the significance of their results, since we could not elaborate." Had Justesen and others been as open in their critique of Merritt's work as they had been in rejecting Frey, this would not have been the case. The same reviewers also claimed that they were not aware of Frey's own criticisms of that work because the latter had not been published in the scientific literature, a shortcoming that was certainly not the result of Frey's unwillingness to debate the issues in print.⁸²

Policy deliberations too have been hampered by the politicization of RF bioeffects research and by the biases and inconsistencies discussed earlier. The knowledge that key decisions on such research have been influenced by persons with vested interests in technological expansion raises questions about the legitimacy of the research. Once the objectivity of science is thrown into doubt, its utility in policymaking is destroyed.

The destructive consequences of politicizing science are well known. Charles Süsskind fully understood what could happen when he argued in 1968, "The lesson is that we must evolve methods of managing and coordinating research without inhibiting the investigator's freedom." He and his colleagues knew that "you must have independent research if you expect to get results that can be believed."⁸³ But despite such warnings politics and science have become intertwined in the microwave debate. The scientific community has allowed social, economic,

and political pressures to influence its activities, thereby destroying the credibility of its product. With the undermining of the credibility of science, the second avenue for resolving the microwave debate (government being the first) became impassable. And perhaps as important the glaring inconsistencies that have emerged over time have brought the mass media and the public into the microwave debate.

8

Mass Media and the Public

What must the press do to get the truth out to the public about low-level radiation? Well, we simply have to keep digging; check, double-check, and if possible triple-check every press release, tip, and press briefing; remain always suspicious of authority, and never lose sight of that old Latin admonition, *Illegitimi non carborundum*—don't let the bastards wear you down.—William Hines, *Annals of the New York Academy of Sciences* (1979)

Some scientists and policymakers who have had to defend past policy decisions during the course of the microwave debate argue that the debate itself was started by and has been kept alive by the mass media and their readers.¹ There is some truth to this argument. Had the press not discovered and publicized the GE television set incident, there is no guarantee that Congress would have acted. Press stories about defective microwave ovens played a crucial role in bringing RF technology under the radiation control umbrella. Coverage of the Moscow embassy radiation problem played a key role in stirring up public interest.

It has also been argued that the main motivation for publicizing the microwave debate has been economic; the microwave story sells newspapers. Again there is some truth to this argument. Popular writers have used the microwave story to sell copy. But there are also more fundamental reasons that account for mass media involvement in the debate. As a managing editor of the Eugene, Oregon, *Register-Guard* explained to readers when defending his decision to print a series of stories about a microwave signal discovered in late 1977:

Notes

Chapter 1

1. "In the Matter of the Application of Home Box Office, Inc.," transcript of hearings held in Rockaway Township, New Jersey, May 1–October 9, 1980, 1:31–32 (hereafter cited as *HBO Hearings*).
2. *Ibid.*, II:81.
3. *Ibid.*
4. *Ibid.*
5. *Ibid.*, II:111.
6. *Ibid.*, III:3.
7. *Ibid.*, V:4–5.
8. *Ibid.*, III:92–93.
9. *Ibid.*, III:111.
10. *Ibid.*, XIII:52–53.
11. *Ibid.*, XI:45.
12. *Ibid.*, XI:76.
13. Paul Brodeur, quoted in Michael Matza, "Microwave Menace," *Boston Phoenix*, February 7, 1978.
14. *Consumer Reports* (March 1973): 221.
15. *Ibid.* (March 1981): 132.
16. Typewritten note (n.d.) by Joe Towne on copies of articles sent to members of the Radar Victims Network, 1976 ff.
17. *FASTA Newsletter* (September 1976).
18. David J. Eisen, director, Research and Information, Newspaper Guild, to me, March 3, 1981.
19. Maurice C. Benewitz, "In the Matter of the Arbitration between Newspaper Guild of New York and the New York Times" (February 1978).
20. *Microwave News* (June 1981): 2.
21. See "Study of RF Sealer Exposures," *Bioelectromagnetics Society Newsletter* (January 1984), p. 7, for recent actions taken in Maryland.

Chapter 2

1. *New York Times*, May 23, 1943, sec. 6, p. 14.
2. W. Holzer and E. Weissenberg, *Foundations of Short Wave Therapy* (London, 1935).
3. H. R. Hosmer, "Heating Effects Observed in a High Frequency Static Field," *Science* 68 (1928): 327.
4. C. M. Carpenter and A. B. Page, "The Production of Fever in Man by Short Radio Waves," *Science* 71 (1930): 450–452.
5. W. H. Bell and D. Ferguson, "Effects of Super-High-Frequency Radio Current on Health of Men Exposed under Service Conditions," *Archives of Physical Therapy, X-Ray, Radium* 12 (1931): 477–490.
6. *Ibid.*, pp. 484–485; cf. R. R. Sayers and D. Harrington, "A Preliminary Study of the Physiological Effects of High Temperatures and High Humidities in Metal Mines," *Public Health Reports* 36 (1921): 116.
7. Bell and Ferguson, "Effects," p. 488.
8. L. E. Daily, "A Clinical Study of the Results of Exposure of Laboratory Personnel to Radar and High Frequency Radio," *U.S. Naval Medical Bulletin* 41 (1943): 1032–1056.
9. B. I. Lidman and C. Cohn, "Effect of Radar Emanations on the Hematopoietic System," *Air Surgeon's Bulletin* 2 (1945): 448–449.
10. F. H. Krusen, J. F. Herrick, U. Leden, and K. G. Wakim, "Microkymatotherapy: Preliminary Report of Experimental Studies of the Heating Effect of Microwaves ("Radar") in Living Tissue," *Proceedings of the Staff Meetings of the Mayo Clinic* 22 (1947): 209–224.
11. S. L. Osborne and J. N. Frederick, "Microwave Radiations: Heating of Human and Animal Tissues by Means of High Frequency Current with Wavelength of Twelve Centimeters (The Microtherm)," *Journal of the American Medical Association* 137 (1948): 1036–1040 (hereafter cited as *JAMA*).
12. A. W. Richardson, T. D. Duane, and H. M. Hines, "Experimental Lenticular Opacities Produced by Microwave Irradiations," *Archives of Physical Medicine* 29 (1948): 765–769.
13. C. J. Imig, J. D. Thomson, and H. M. Hines, "Testicular Degeneration as a Result of Microwave Irradiation," *Proceedings of the Society of Experimental Biology and Medicine* 69 (1948): 382–386.
14. J. T. McLaughlin, "A Study of Possible Health Hazards from Exposure to Microwave Radiation," unpublished ms. (Culver City, Calif.: Hughes Aircraft, February 9, 1953). For McLaughlin's comments on the origins of this report, see "Biological Effects of Microwaves," minutes from a Navy Department Conference, Naval Medical Research Institute, Bethesda, Md., April 29, 1953, pp. 5–8 (hereafter cited as Navy Conference [1953]).
15. Navy Conference (1953), pp. 15–16.
16. *Ibid.*, p. 16.
17. "Radar and Cataracts," *JAMA* 150 (1952): 528.
18. F. G. Hirsh and J. T. Parker, "Bilateral Lenticular Opacities Occurring in